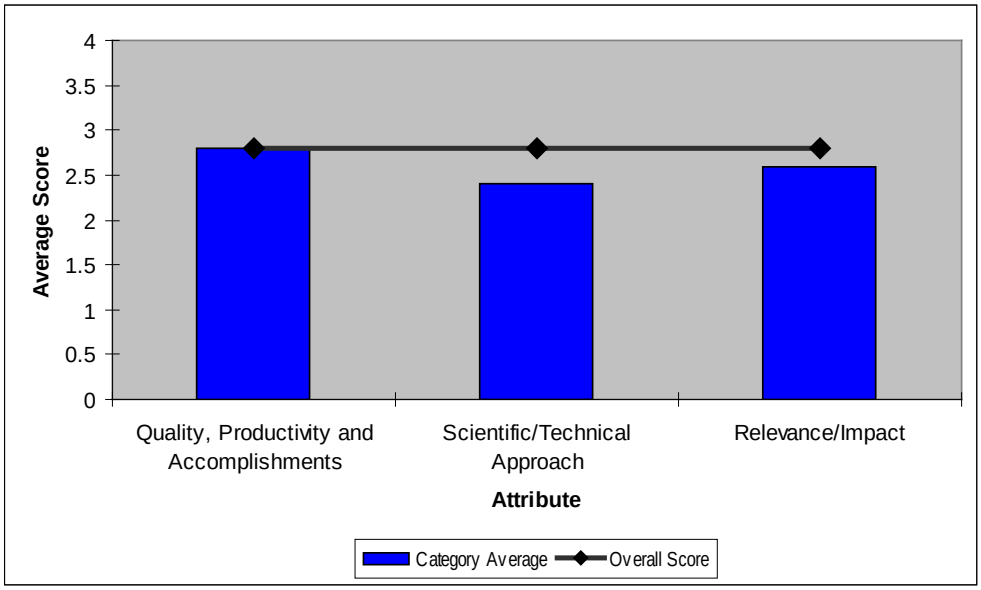
Improved Electrodes and Electrolytes for Dye-Based Solar Cells

Principal Investigator: ***Harry Allcock, Pennsylvania State University***



Quality, Productivity and Accomplishments (Average Rating 2.8)

Rating	Comments
3.0	Good university study of the DSSC. Work is well grounded in a multi-disciplinary team. Synthetic chemistry aspects are strong as are the synthesis of the nanostructures.
2.2	The stated objectives for this program may be consistent with the SETP goals, but should be made more specific to the proposed work itself and how this work will enable high efficiency, next generation PV design. The objectives should be forward pointing, is this work exploratory in nature? The PI should not settle for the generic default. Part of the work was developed through a shared postdoctoral appointment, based upon technologies originating the Craig Grimes' work in a separate program. The other research progress has tended to follow the general direction of the SETP program; however, they are individual, non-integrated results. Results to date would suggest that the performance of the trial system was not outstanding in comparison with other related systems. NREL-calibrated measurements of system performance are needed to be able to make more appropriate comparisons across technologies. **Qualifications of Research Team and Available Resources:** Professors Allcock, Mallouk, Horn, and Grimes are all senior researchers with outstanding research track records, with responsibilities corresponding to their areas of strength. Facilities adequate to the proposed research.
3.0	Allcock/Mallouk/Grimes et al. are an excellent team with impeccable credentials to work in this area. The 10 month progress has included non-volatile electrolytes, high surface area TiO2 electrodes and the assembly of polymer devices with an efficiency of 3% compared to 1% for other polymer systems. The problem appears to be an old one- penertration of the polymer conductor into porous TiO2 structures. The phosphazine polymers developed here do not appear to address this problem. Since iodine is still required, it is unclear how variations of the approach will achieve efficiencies

above the state of the art going forward.

3.0 Strong track record of accomplishment, superb chemistry being done, but is this the right approach to solve these problems? The polymer chemistry is at the forefront and if the P.I. wanted to engage more in out of the box concepts, would likely lead to new technologies.

3.0 The team looks solid with some initial results.

Scientific/Technical Approach (Average Rating 2.4)

Rating	Comments
2.0	This is worth studying. It could be the last ditch hope for DSSC. The researchers in the project are device oriented. That is good. Unfortunately, the efficiency is still low and they still need to prove that the approach solves the key challenge for DSSCs – longevity and stability enough to meet LCOE goals. That comes next. Unfortunately, gel electrolytes and nanostructured TiO2 produced similar efficiencies (3%) and even the preliminary work here shows that it is unlikely that the Allcock group will not be able to break through this limitation. The researchers should be able to calculate the diffusion coefficient in their polymer and compare it to what is necessary to have an efficient DSSC (comparing it to the values in pure liquid). The researchers should also do control experiments with liquid electrolytes and gel electrolytes with flat interfaces. The efficiency results should be studied as a function of light intensity. One must test also whether or not the central premise that the system can be stable.
2.2	The task list for this program is again consistent with the SETP target objectives, but unspecific in details in their approach to address the fundamental questions of improved performance (how do we get to 10%) and longevity (10 years?). The uniqueness of the portions of the system for this particular application is in question, the polymer system was developed a number of years ago for battery-related applications (and patented?). It was expected that further details on selection of new polymer systems which could also act as the electrolyte and could further improve electrode contact would have been provided. Two of the historic barriers for portions of this design into new technologies have been its cost and the availability of raw materials. Both issues will bring challenges to scale up efforts, and should be addressed comparative to other dye-based technologies.
2.0	This process is good direction to go in. Good penetration of the polymer into the nanostructured charge conducting phase does not appear to have been demonstrated, however. Future proposed work may indeed improve the cell efficiency although, as with other projects, it is important that such improvements be compared with the achievable state-of-the-art cell rather than with a low-efficiency starting point. It is difficult to see the long-term stability problems ever being resolved without certain changes in this process. It would have been nice to have seen proposals on how to further develop the concept to improve the polymer properties.
3.0	Very focused approach on a new electrolyte. This project seems to be innovative, not sure if it will work, but at least it is a new approach. However, we are still stuck a conventional electrolyte? Why not combine this approach with a real “hole-transport” polymer? Seems like innovative chemistry being used to “engineer” a DSSC technology which has been fundamentally flawed from the beginning?
3.0	Looks like valuable basic research. Research direction well justified. But devices do not look competitive.

Relevance/Impact (Average Rating 2.6)

Rating	Comments
2.0	One can already see that the longevity and efficiency may not be what is needed to establish

favorable LCOE for DSSC. That information would be something that could/should be established in the next 6 to 9 months of the project. If the answer is no, then it means that only a solid-state approach is viable for DSSC. There is at least one other project in the DOE portfolio that goes in this direction.

2.2 The presentation provided insufficient details to estimate the probability of this program having significant exploratory impact for the SETP program. The research program has identified a number of technical issues, which if resolved, could provide useful technologies. Whether or not this technology is a system solution or a compatible part of another solution is not clear at this stage of development. Research seems generic, but there are two areas not resolved; longevity and the specific potential of the system. The PI's need to better assess how these activities address significant technical or market barriers in more than general terms. Publication route for dissemination of knowledge likely, and in high quality technical journals. Path to technology transfer is unclear, and could be problematic considering the state of the resources being used for this design.

2.5 Another 2.5 rating here- it's hard to say the rate of accomplishments has been slow because the project has been funded for a relatively short period of time and the accomplishments are, in fact, significant in that a new type of polymer has been demonstrated. In terms of progress towards DOE goals, however, this has only been demonstrated by comparison with a very poor polymer device and it was not made clear exactly what path will be followed to reach the actual DOE goals. It is therefore correspondingly difficult to assert that the results represent "significant progress" towards these goals.

Several other groups are working on these electrodes and an effective way to get the polymer and/or electrolyte intimately diffused into the conductive phase has not yet been found. If the PIs could achieve an efficient, all-polymer DSSC system (efficient being defined as above 6%, for example) then the impact would be significant. Alternatively, if they could eliminate a certain portion of the couple without significantly impacting the efficiency, that would also be impactful. As it stands, however, the likelihood seems to be for only small incremental improvements.

3.5 Seems to address a very important problem which leads to demise of DSSCs. The problem of wettability is quite important and seems to be a show stopper. Why aren't there more ideas vetted here about what approaches will be taken?

3.0 How competitive is this work with Gratzel? What possibility is there of approaching >10% efficiency?

Overall (Average Rating 2.8)

Rating Comments

2.0 One should then quickly expand or drop the DSSC from U.S. DOE funding goals. It's being worked on elsewhere in the world and they are far ahead at answering the question of whether this 10 + year plus technology is viable.

3.2 This is a good program with the potential to achieve greater impact factors with a better focus on the relationship of the research technology to the exploratory research needs. The development of non-volatile electrolytes and improved electrodes will be a persistent need area for solar cells now and in the 2015 SAI horizon. The question that the PI's need to address is whether they would prefer to address the pre-2015 or post-2015 needs. Solid electrolyte systems are a solid focus area, and their efforts should include an estimate of the time progression of efficiency improvements. Corresponding device measurements will need a comparative basis to be able to judge progress with contemporary technologies. It is unclear how far the PI's intend to pursue certain activities, and what strategy will be employed to better design the polymer electrolyte system (besides the gel strategy). Is the demonstrated technology the electrode technology of the future – how far into the

future – and how is the solid electrolyte strategy going to address changing materials technologies on the electrode surface?

3.0 I think this project has made a worthy start and is exploring a new area of molecular space (for DSSCs), but the U.S. is clearly lagging behind in DSSC research and the pressure is on to show some continuous and significant improvement relative to state of the art in the next year, with subsequent clear and hypothesis-driven plans for how to take the lead in at least one area of DSSC research.

3.0 Have good polymer, have good oriented nanotubes, need to bring these together!! Why not use a completely different, "bottom up" approach? This is a team of very good chemists, which might be able to take this much farther and should be encouraged to do so.

na This project does not paint an optimistic picture of the state of DSSC technology. The $/W projections from 2004 show DSSC cheaper than competition. But in the meantime CdTe improved to <$1/W, and others have improved as well. But has DSSC technology advanced also?